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BY

J. LAURENCE PRITCHARD, Associate Fellow.

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Notices of the Royal Aëronautical Society.

New List of Members.

It is proposed shortly to issue a new list of members of the Society. The work will be greatly facilitated if all members will communicate to the Secretary details of any honours, degrees, etc., to which they may be entitled, together with their permanent addresses. It is very desirable to make this record as complete and accurate as possible.

Cape Branch of the Aëronautical Society of South Africa.

A communication has been received from Mr. J. I. Hartley regarding the future of the Cape Branch of the Aëronautical Society of South Africa, as the post of Secretary has remained vacant since the death of Mr. Hartley's father. He is anxious to get in touch with Major A. M. Rogers, R.E., Chairman of this branch. If any member has knowledge of the whereabouts of Major Rogers, if he will communicate with the Secretary the information will be forwarded to Mr. Hartley.

Library.

It is desired to appeal to members to present to the Society copies of any books which they may write, for inclusion in the Library. This is at present dependent upon gifts and copies forwarded by publishers for review in the Journal. It is felt that if every member, on the production of a book, were to present a copy to the Society there would be less fear of the Library becoming out of date, which is a real danger if reliance is placed merely upon review copies and occasional benefactions.

The Library is now open for the use of members on Saturday afternoons between the hours of 2 and 5 p.m.

Transactions of the Society.

The next publication in the "Transactions" of the Society will be a paper by Major L. N. G. Filon, Fellow, and Commander Baker, on "Aerial Navigation," which is now in the hands of the printers. The "Transactions" are intended to embody original papers of a fundamental character in the science and

technology of aeronautics. Each paper will be reported on by "referees" appointed by the Council before publication, and they will be issued from time to time wholly independently of the Journal, which will continue to contain notes on the Society's activities, reports of lectures, and original papers of a less fundamental character than those in the "Transactions," in addition to reviews of new publications, etc. Contributions to the Journal are invited from members.

Forthcoming Arrangements.

Among the lectures of the coming Session will be papers by the following members:—

Jan. 21st.—Mr. A. P. Cole. Subject: "The Principles of Rigid Airship Construction." Chairman: Wing Commander T. R. Cave-Browne-Cave, C.B.E.

Feb. 4th.—Squadron Leader J. E. M. Pritchard, O.B.E. Subject: "Rigid Airships and their Probable Lines of Development."

Feb. 18th.—Squadron Leader Percy Bishop, O.B.E., Associate Fellow. Subject: "Aircraft Design in Relation to Standardisation." Chairman: Mr. H. White Smith.

April 14th.—Brigadier-General Lord Montagu of Beaulieu, C.S.I., C.C., J.P., V.D., D.L., M.P. Subject: "A Comparison of the Cost of Air Ton Miles Compared with other Forms of Transport."

May 26th.—Sir Richard Glazebrook, K.C.B., F.R.S., Fellow. Subject: "Some Points of Importance in the Work of the Advisory Committee for Aeronautics."

Other papers, of which the dates are not yet definitely fixed, will be read by Prof. B. Melville Jones on "Flying over Clouds in Relation to Commercial Aeronautics," and "Aerial Machinery" by Major C. F. Abell, O.B.E.

Editor of the Journal.

The Council have appointed Mr. J. Laurence Pritchard, Associate Fellow, to act as Editor of the Journal.

Correction.

The name of the expert on birds in Sir Horace Darwin's paper should be "Gadow" and not as printed on page 620 of the Journal.

W. Lockwood Marsh,
Secretary.

PROCEEDINGS.

SECOND MEETING, 55th SESSION.

The Second Meeting of the Fifty-Fifth Session was held in the Hall of the Royal Society of Arts, London, on Wednesday, November 12, 1919, Brigadier-General R. K. Bagnall Wild (Chairman of the Society) presiding.

The CHAIRMAN said he regretted to have to announce that Sir Arbuthnot Lane had been called away to the country to perform an operation, and was unable to take the chair on that occasion, and he would do his best to take his place. He had pleasure in calling upon Dr. Atkin Swan for his Lecture.

Dr. CHAS. ATKIN SWAN, L.R.C.P., M.R.C.S., M.B., Ch.M., F.R.G.S., F.R.P.S., then delivered the following Lecture:—

SOME PHYSICAL AND PSYCHICAL EFFECTS OF ALTITUDE.

MR. CHAIRMAN AND GENTLEMEN,

When some twenty-five years ago I began making notes of the effects of altitude on the human animal, I never dreamt I should be called upon to make public any of my observations; I am doing so now with the utmost diffidence and buoyed up by the hope that a few of the facts I am about to bring to your notice may be even indirectly useful.

In view of the rapidly approaching use of commercial aviation as a means of passenger transport, I wish it to be clearly understood that I am not approaching the subject from the purely scientific point of view, but rather from the standpoint of the men in the street, whose experience of altitude may be very moderate, as indeed is my own. Seeing that from 10,000 to 15,000 feet is the average height at which I made my observations, partly for convenience sake and partly as I found those altitudes were conducive to comfort; and I, therefore, have little or nothing to say about that valuable invention, the low pressure chamber or the apparatus for rebreathing experiments, or on the use of oxygen for inhalation, which latter has, of course, been invaluable or rather a necessity when dealing with altitudes above about 18,000 feet. I am rather attempting to show how the ordinary mortal can get on, and very well too, at the moderate altitudes I selected as the grounds on which I made my own experiments; and to try and analyse as far as I can from my own researches, and aided by those of far greater people than myself, so as to find out what are the causes of discomforts at those altitudes, and what is far more important, how they can be avoided.

Looking back to the early history of aviation I do not know that I derived much information that was useful, for on studying the account given by Dr. Jeffries when he crossed from Dover to the forest near Calais in 1785, the only discomfort upon which he laid stress is that he suffered severely from cold, and this is hardly a matter for astonishment, as he states plainly that owing to the small amount of ballast carried he and his companion had literally to divest themselves of every shred of clothing they possessed, so that on their arrival their warmth was only restored to them by the kindness of the ladies who rode out from the chateau to meet them and lent them their cloaks as protection during the eleven mile ride on horseback to the chateau; also the height attained was apparently only 1,000 feet or two.

Possibly we can all agree that the chief factors causing physiological or pathological phenomena (I am using those terms clumsily, meaning to distinguish between effects that are either within or without the bounds of health) at various altitudes are the chemical differences in the atmosphere and the various in pressure. I have tried to separate the physical and psychical causes and effects as far as possible, but I cannot say I have been successful, as the two appear to be inseparably mingled. If I appear to be stating facts that are too evident to everyone, I must ask you to pardon me as this short paper is addressed not only to the scientific public.

There is no doubt that at about sea-level we deal with a pressure of 760 m.m. of mercury and a comfortable atmosphere of, say, about 20 per cent. of oxygen. At 6,000 feet we have roughly a pressure of 615 m.m. and about 16 per cent. of oxygen; at 14,000 about 450 m.m. and about 12 per cent. of oxygen. Equally we agree that as we ascend the higher pressure gradually merges into the lower. These chemical and physical changes may, of course, account for many of the phenomena, and possibly by mal-nutrition may upset the nervous system, and at any rate help to produce the physical effects, though in the Alpine experiments there are obviously other causes, such as the emotional effect of the scenery, the brilliant sun or driving snow, as the case may be; the delightful clear air, or on the other hand, dense fog.

I must here apologise for the egotistical nature of a great deal of this paper, as during my early experiments I was working alone in the mountains and for obvious reasons did not take my subjects into my confidence, but always made photography my excuse for the various ascents; and after 1914 by far the larger part of my work was carried out in the early dates of the war, before the R.A.F., M.C., was in being in this country, as far as research went; and I would like to take this opportunity of offering my tribute of respect and admiration for the researches carried out by that untiring body of scientists, and also to men like Colonel Lister and Colonel Jones, of the United States Army, who were kind enough to make use of some of the work I had carried out.

If you will not be too bored I propose to, first of all, give you a few facts and data, as far as I was able to collect them, concerning happenings on various mountains in Switzerland and the Dolomites where muscular effort was involved, then to compare the results of investigations on people of a very different type, who ascended from 1,800 feet to 11,100 by train. Then I have a few words to say, chiefly, as to where I had gone off on false trails in experiments made in the early days of the war; and lastly, I would ask you to hear a few instances of how "staleness" in pilots has been curtailed by drugs, though I have not the slightest doubt that you will all be very sceptical as to the truth of these latter statements.

In my first set of experiments I kept as closely as I could to two different types of ascent:—(*a*) Where there was nothing sensational or heady, but simply a steady climb; (*b*) where the ascents involved traverses with little or no help here and there save what was afforded by the rope; this was done purposely in order to bring in the emotional side of the experiment.

It would seem that there is a definite altitude for each individual where the low atmospheric pressure or the deficiency in oxygen caused so much disturbance, that what I would term the compensating mechanism seemed to be unable to adjust the organs to the changed conditions; and all through the experiments I hope and believe you will notice that it is not so much the changes that matter as the power of the individual to adjust himself or herself to them.

Even at so low an altitude as 7,000 feet I found a large number suffering dyspnoea slightly at first, but increasingly so for the first four or five days, after which recovery took place. This was as it should be according to the text books, but, as we shall see, there are many cases where the sequence of symptoms are reversed.

One thing struck me, and that was the same individuals had what one might

term a "relapse" when either fog or really strong wind came on, and this would happen without their complicating matters by taking exercise. Of course I am not including any of those health resorts, such as Arosa, where the bulk of the tourists are presumably patients suffering from lung trouble, but I am dealing with what are supposed to be the ordinary healthy adults.

Several times I was greatly struck by the fact that the dyspnoea was greatest at night, about 2-4 a.m., when of course vitality is at its lowest end; and one little instance may be interesting:—I was going to my room rather late—about 2.30 a.m.—when I noticed the door of a bedroom open and sounds from within as though the occupant was not altogether happy. I cautiously peeped in and found a lady, apparently about 30, lying on her back; her lips and finger nails dusky; her pulse well over 130 and her respiration about 34. I gave her a dose of oil of peppermint and salol and she rapidly recovered, and then told me that she and her brother had come straight up from Genoa and had gone to bed feeling tired; she had woke up or rather had been wakened by a feeling of oppression in her chest. I went into the brother's room and found him in much the same condition, and gave him a dose of whisky and some strychnine; he suffered more or less all night while the sister slept perfectly happily; the next day they both went for a walk, danced in the evening, went to bed and had no further trouble. The comparison of these two cases seemed to me to point to the splanchnics being at fault, or at any rate being responsible for some part of the trouble. I examined both a day after. Pulse 72-76; respiration 18; absolutely no symptoms. They remained the whole season at from 8,000 to 10,000 feet without ill effects. Clearly the recovery was too rapid for the malady to have been true mountain or altitude sickness, yet at first sight it seemed a clear case.

An ascent up to 13,400 feet was rather interesting. We mounted slowly (1,000 feet per hour). Out of a party of ten, five had no symptoms at all; pulse 120 to 125; respirations 20 to 24; three vomited; pulse 120 to 130; respirations 28. These three complained of what they termed misty vision and showed a certain amount of cyanosis; two suffered a certain amount of dyspnoea; pulse 120; respirations about 30; very slight cyanosis, and black coffee apparently set them right. Two were perfectly well on the ascent, but on going down complained of slight frontal headache, and curiously enough, although their pulses and respirations were respectively about 120 and 30, they complained of no distress until near the hut on the descent, when they described their feelings as breathing air that was stuffy; severe frontal headache set in; aspirin increased it, but salol effected the recovery within an hour. The next day everybody was well, and we repeated the same ascent but by another route. I alone carried the barometer, instruments and other medical comforts, so no one knew at what altitude they were, but the same symptoms were repeated within 100 feet of the same altitudes as on the previous day.

Repeated these experiments several times, always on subjects who had not made ascents before; but as far as possible I avoided complications in the way of unusual fatigue, as each member of the party was physically fit and able to walk for considerably longer periods than the time of climbing, so as to allow for additional fatigue during the ascent. One curious point is worth mentioning. When *walking* up a slope caused distress, as soon as the gradient became steep enough to allow of our going on all fours, everyone moved on with much greater comfort. Also on one occasion another point came to light. We had been accompanied one year by an Irish terrier dog, who became exceedingly expert in climbing, and one day at the top of a peak, about 14,000 feet, the dog was perfectly happy; without thinking I held him up by his fore paws as though he was begging; he began to pant and the inside of his lips became darker in colour; I let him go and he soon recovered. His master rather doubting the truth of what I said about the dog called him to him and repeated the same experiment with exactly the same result. It was obvious in this case that the change of posture had done something to disturb the animal's sympathetic nerves. I should

add that the halt at the end of each ascent was about 15 to 30 minutes, so as to regularise any symptoms during the descent.

I had only a primitive manometer in those days with a closed end. As far as I could vouch for its accuracy, which I fear was not very great, the systolic pressure before commencing the ascent varied from 108 to 120 m.m. In people who suffered no distress at all there was a rise of about 5 to 10 m.m. in the two people who vomited. The rise was shortly after followed by a fall. The interesting point was that whether these were symptoms or not, the rise of B.P. was much the same, as if the symptoms bore no ratio to the sensitiveness of the medullary centres.

I regret I had no opportunity of repeating the tests at the conclusion of the climb to see when the normal B.P. was reached.

All these cases seemed to point to a definite physical cause disturbing the machinery during the experiments, but to my surprise when making another test up to only 9,000 feet on the glacier side of the Riffelhorn, which as you all know is a fairly vertical climb, although only about 2,000 feet, as we start at 7,000, practically the same phenomena can be produced—I take it, chiefly due to mental effect. In no case was anyone the worse the next day, and the same experiments were repeated practically every day for a week with very little alteration in physical signs.

I regret that I am not able to give any data as to how far the two who vomited were able to recover their power of compensation, as they gave up any further attempts, but I heard a week afterwards they were none the worse.

Were it not for wearying you I could give many more similar instances on Monte Rosa, 15,000 feet, and the Gabelhorn, about 14,000 feet. The general result seemed to be increase of pulse rate and respirations, and a rise of B.P. all round, the only difference between what I should call fit and unfit subjects being distress on ascending, and frontal headache on the descent, with the curious addition that in some cases the rise of B.P. was followed by a fall.

In support of the oxygen-hunger point of view I might mention an interesting little incident. I was at about 6,000 feet about to start on a short, ordinary ascent. Outside the hotel was a boulder, too smooth for ordinary climbing, it was only about 60 feet high. A chaplain with me was anxious to get up to the top. As I had my rope-shoes on I offered to get him up with the help of one of the guides. We roped up in a hurry over one shoulder. I went on, and at the top hauled on the rope. Up came the Chaplain, and we were much interested to find that he was absolutely blue, and practically unconscious. His respirations were 38, pulse 130, and he was showing every sign of oxygen-hunger. On examination we found that in my haste and being very cold, instead of using a bowline I had tied him up in a "granny," which of course slipped and, compressing his wind-pipe, had given him a delightful oxygen-hunger. It happened that there was a lady at the hotel suffering from pneumonia, and was being treated with oxygen. We merely had to put the tube between his lips, and he was none the worse for this adventure.

This, I think, illustrates very clearly that oxygen-hunger alone will produce the symptons we have been discussing.

In all I made 104 observations. In two there was a transitory albuminuria. I happened to be consulted by some eight or ten at the close of the season and in about four cases there was marked phospaturia and oxaluria; the former I put down to the effects of strain, the latter I must confess puzzles me altogether. In all those cases which I would term slight and acute cases I almost invariably found that the old guides' remedy of oil of cinnamon on sugar gave great relief, as if gastric and intestinal distension were the first cause of the trouble while the circulatory and other essential compensating changes were quietly taking place meanwhile.

Happening to make a test on a rock climb at about 12,000 feet on a subject, a Mr. X——, who had shown no distress whatever, I was able to obtain all the

physical signs as well as symptoms of mountain sickness simply by letting the subject hang over the edge of an arrête, and to eliminate the altitude I repeated the same experiments on a needle of rock, the summit of which was barely 5,000 feet above sea-level, with exactly the same results—rapid pulse, rapid breathing and subsequent headache, but hardly any cyanosis, but marked rise of blood pressure immediately afterwards.

When the train ran up the Jungfrau, up to 11,000 feet it was easy to make numberless experiments. It was usual to start at an altitude of 1,800 feet and go practically straight up to 11,000 or more in about two hours.

If you will allow me I will give you some of the results during two weeks' observation; naturally I excluded all obviously unfit subjects, such as very elderly people. Here you will see there was no question of muscular effort complicating matters, save in the last experiment. Out of 72 subjects examined the pulse was raised by about 5 to 20 beats, respirations varied greatly from 22 to 38; as far as my rough manometer allowed I estimated the rise at about 10 m.m. I was able to count about 600 that I could keep fairly well under observation without their being aware of it. There was not a single case of hemorrhage, only 9 vomited, 23 showed some little distress on walking at the summit, no one seemed to suffer from troubles of vision, but were able to take photos with comfort. Of the 72 I was only able to find 10 who complained of tinnitus, and my private opinion is that many of those ten said they did because they thought it was the correct thing to say. In any case everyone was relieved by swallowing, save one wretched youth who had obvious adenoids. A good many passengers complained of sore throats, but this may possibly have been due to the extreme dryness of the air, and wind coming off the ice.

I made several experiments with regard to B.P. on people who stayed at the summit about 12 hours. Out of about 40, 19 showed distinct fall of B.P., and all the way down in the train they were irritable, becoming worse on descending. Some few complained of headaches. I was able to ascertain that everyone was entirely well the next day. Also after repeated ascents, going up and down the whole day, the B.P. at the end of the day was below normal in 12, and up in 4 or 5, and out of 17, four complained of headaches, and they were all more or less irritable. There was one additional experiment on a party of five; part of the results landed me in a hopeless puzzle. We went up by train to 11,000 feet and then climbed to the summit, or rather walked as it cannot be called a climb; pulse and respirations increased only by about ten and two respectively; B.P. I was not able to take. There were no symptoms at all save in one man who had been perfectly fit, but at 12,500, roughly, became suddenly cyanosed; his pulse was rapid 140 and wiry; he had to be helped down. I afterwards learnt that he was a strong positive Wassermann.

There were two old ladies, apparently over 60 and very stout; to my disgust they neither of them suffered on the ascent or descent. I had a little chat with them in the evening, and they told me they had just come from Carlsbad.

One other very curious point came out, about five years ago, in a little quiet mountain place with no excitement there to raise possible complications. There were two young adults who had just arrived, and on the same day they both had what seemed to be an endless succession of epileptic seizures, although they had suffered none for more than ten years.

I came across the case of a man who was presumably perfectly sound, at least he had been examined frequently and told so. After two months of passes, varying from 8,000 to 14,000, he had a marked systolic murmur, but suffered no symptoms.

It has often struck me that what may be termed the reaction time in guides is slow and their movements needlessly deliberate. This, of course, may be due entirely to their training, but they show it even in their games during the season, and if one meets them at other periods of the year, down in the valleys, they

seem quite different in temperament, I mean much more alive and brisk, physically and mentally.

I have tried thus far to examine the various different conditions which take place at high altitudes, such as the low pressure and partial oxygen pressure; there is also the cold to be considered, often very strong wind, and although it may seem fanciful, I always think there is some form of electric tension in air which causes the general exhilaration; when we have all these causes together some of the signs and symptoms are almost contradictory.

If you will allow me, I will give you the experiences of a man we will call X. He was a person of what I call low mentality and really had not sufficient imagination to understand that he would damage himself if he fell from the rope. Giddiness, in the ordinary sense of the word, was a thing he could not grasp, so we may take it he was a good subject for a test by all various conditions together. He made the ascent of the Gabelhorn (14,700 feet) in mid-September, after having been in the valley at an altitude of about 1,000 feet for a week or two. The ascent and descent took altogether about 14 hours. He suffered dyspnoea, but no actual pain, though conscious of rapid breathing as soon as he passed 11,000 feet. Owing to the lateness of the season and to the weather not being good, cold was of course considerable. At about 13,000 he began to suffer vertigo; pulse was rapid and seemed to him thundering in his ears; he was told it was snowing, but that did not worry him; he felt a keen desire to go on, but his feet and hands were heavy, this was when walking; on reaching the rocks about 13,000, he vomited, then climbed with much greater comfort on all fours, but at the summit the difficulty in breathing returned. He seemed to have voluntarily resorted to what is known as Cheyne Stokes breathing. Coming down the same form of breathing returned at intervals, involuntarily; his B.P. was roughly 15 below normal. The next day he was none the worse; B.P. normal; age 46. I attributed his getting off so lightly to the fact that on the ascent more than half his food was lost, so his digestive organs were not over-taxed. Examined next day, pulse 140, respirations 28, no pain or discomfort and no more tired than after an ascent by train to 11,000. This seems to point to the fact that compensation takes place more or less rapidly in different people, but that either muscular fatigue or too long a continuance at a particular altitude that is high for each particular individual may cause a reversal of the usual phenomena; in other words, instead of muscular effort stimulating the medullary centres, it may help to exhaust them rapidly.

Putting all these facts together it seems that in a case where muscular effort is employed the rise of pulse rate, respirations and B.P. are very little in excess in those cases where there are symptoms of distress over those when no distress is felt. It also seems to me to draw attention to the very large part played by digestive organs, and also by the higher nerve centres, as shown in cases where a little sensation is introduced, for in this case I should have mentioned that the B.P. rose in much greater ratio, also the headache and fatigue in the evening were just as marked as after the more fatiguing ascent. Of course I do not mean for one moment to imply that because all the signs and symptoms can be produced without any great actual altitude, therefore we can afford to ignore the physical changes caused by those high altitudes, but merely to point out that it is not fair to blame altitude only for all the discomfort.

In trying to estimate the factors for these physiological effects I have begun by examining the effects of want of oxygen resulting from lower pressure of this gas. Dr. Ravenhill has described two types of altitude sickness, one the acute as in most of my experiments, and secondly, the slow, beginning often with cyanosis, but it seems to me that the slow form is more what I would term a throwing up of the sponge by a compensating mechanism, though the two are often shaded into each other.

Dr. Ravenhill has divided the slow cases into two; one in which the cardiac symptoms are greatest, (2) in which the nervous symptoms predominate. We

know that given a supply of oxygen in the blood it is taken up at once by tissues which apparently take what they need on account of the higher pressure of oxygen in hæmoglobin, also seeing that the nervous centres are especially sensitive to want of oxygen, being first stimulated and then depressed. I would hazard the suggestion that this may partially explain the rise and subsequent fall due to fatigue of the vaso-motor centre; and also this may help to raise the gastric symptoms which I have tried to show are very common.

I hope you will agree that the experiments I have described fairly prove that a compensating mechanism does exist and does its work well. There were some interesting facts brought to light during experiments on B.P., carried out by myself and Mr. Richard Lake at various aerodromes. Thirty-eight tested at Gosport, about 5,000 feet, 10 m.m. average rise; 50 at Wyton, same altitude, about 10 m.m. rise. More than half showed a slight fall, about 6 m.m.

A great deal of work was being done at this aerodrome and most of the subjects were pretty tired when tested; 70 at Dover, 40,000 feet, average rise of 5 m.m.; 30 at North Halt, most of the subjects had been experimented on with the revolving chair before flying. Nineteen showed a fall of a few m.m.s, the rest a small rise. Forty-two tested at Hounslow, altitudes 4,000 to 6,000, and average rise of 10 m.m., save Major Joscelyn Swan, who has allowed me to mention his case, was taken up to 6,000 feet and came down in about an hour; before ascent his maximum B.P. was 120, after flight 80. He complained of severe occipital headaches afterwards. Three who showed a distinct fall after flying suffered from astigmatism.

I wonder if you will allow me, in conclusion, to say a few words concerning treatment of "staleness" in pilots. In the early days of the war I had pilots sent me for treatment who had gone wrong and could not fly. I asked the authorities if they could give me any help, and the only answer at the beginning of 1915 was that it was a malady caused by a low temperature of the lowest extremities due to strong vertical up draught. This of course was definite, but not much help in repairing the pilot.

Rightly or wrongly, it seemed to me that the trouble was due to fatigue of the mechanism for acclimatisation as the pilot was always ascending or descending. Therefore whatever mechanism there was, possibly respiratory and vaso-motor centres, the mechanism was overworked, and also the constant blood changes damaged, for the time being, the nutrition of the whole nervous system. Rest was the first obvious point in treatment.

I next tried to help them by sterilising the digestive tract and then improving the general nerve tone by intramuscular injections.

I came across a very few cases of persistent low B.P. In these I tried the experiment of injecting ergotin with a view to reassuring the patient and showing him that this trouble could be overcome, although, of course, it is not to be supposed that one could by any drug raise the B.P. permanently. This gave the desired mental support and enabled me to carry out my definite treatment. A large number of cases ended successfully and resumed aviation.

I need hardly say that all outside causes of fatigue, such as eye strain, bad condition of gums, setting up asepsis, blocked nostrils, and lastly (as far as possible) mental worry were removed; it must of course be remembered that a pilot, flying under war conditions, drove a fast climbing machine which put an entirely different strain upon his compensating mechanism to that required for ordinary commercial aviation. But in spite of this, what a small proportion of pilots came to grief on account of troubles due to altitude only, and what is more astonishing, how many flew successfully who by all rules of the game should have been unfit for aviation at all. A goodly number of pre-systolic murmurs spent many hours high in the air for many months and were none the worse afterwards.

In conclusion, I would like to mention the occasion when Major Lloyd took up the gentleman who makes the manometers that we all use. At my request he gave

him a most blood-curdling nose dive, and then flattened out at the very last moment. His blood pressure was 124 when we started and when we got in again it was 190.

I hope that what I have said this evening will go some way towards convincing the laity that most of us can take advantage of aviation as a means of transport, and should we ever feel unwell during our air excursions let us remember (1) the adaptability of our systems; (2) that far the larger part of the discomforts of this life are due to digestion; that those of us who are sea-sick get over it and are none the worse, therefore there is no reason why we should not get over air-sickness. And lastly, that we should be chary of putting down every discomfort we notice when at an altitude of 12,000 or 14,000 feet to that altitude, remembering that if our car refuses to move when travelling on an unaccustomed road we do not say the road is impossible, but rather examine our engine, see what is amiss in each particular case. Is it not common justice to accord the same common-sense leniency to our bodies, seeing how much we all want to take them by air?

DISCUSSION.

Major-General R. BROOKE-POPHAM said he had met Dr. Swan under three different conditions. The first time was early in 1915 when he reported to him for medical advice—he thought he was excused the buff slip. After examination, he gave him a bottle of medicine, and as it was not so nasty to drink as it looked, he followed the directions, and to his surprise (he did not know Dr. Swan as well then as he did now) he rapidly got better. The next time he met him was when they were in considerable difficulties over in France in regard to photography. It involved obscure optical and chemical troubles. They referred them to Dr. Swan and again the difficulty was got over. As a result of meeting Dr. Swan on those two occasions, he came to the conclusion that he was not only a great scientist with vast experience, but a man who, if he took up a subject, would never rest until he got down to bedrock facts and found out the causes of all the phenomena connected with the subject. The opinion he then formed had been nowise altered by listening to what he had said that evening. The Lecture had greatly advanced their knowledge of the subject. He was sorry he had not mentioned oxygen. They attached vast importance to that in France, and he thought they got a vast amount of benefit from it. Dr. Swan rather suggested, he thought, that oxygen would not be required for commercial flying, but he was not sure of that. Commercial flying at present was generally carried out at altitudes of 5,000 or 6,000 feet, but when they were able to deal with meteorology to a greater extent than at present and knew more about the winds at high altitudes, he thought commercial aviation would take advantage of that knowledge and fly at different altitudes, so as to get the advantage of the wind. They might have commercial flying at 20,000 feet, in which case oxygen would be a necessity. In any case he would like to ask Dr. Swan whether it was not advisable for those interested in commercial aviation to study the question of oxygen, because it seemed that a good many people would be affected by comparatively low altitudes, perhaps 10,000 or 12,000 feet, and, although it might comfort them to know they were going to recover from it eventually, it would increase their happiness at the time if they were given something that would avoid any objectionable symptoms. Would it not be desirable to carry oxygen for those people who were likely to suffer at comparatively low altitudes?

Colonel WAKEFIELD said he was afraid he could not contribute any useful remarks on the subject. He could only state that when he was a patient of Dr. Swan's (he thought it was in 1915), he suffered with what he thought Dr. Swan called gastric trouble. He went back to France the next year and put in about a couple of thousand hours flying altogether. He was only demobilised two days ago. He had known very few cases where people had suffered from comparatively

low altitudes, such as 10,000 feet, but if great heights such as 20,000 feet were reached in commercial aviation, as General Brooke-Popham thought possible, that would introduce a different problem.

Dr. L. E. STAMM thanked Dr. Swan for his interesting Paper, and said in scientific work of that sort a Paper like that gave a very feeble representation of the amount of work involved in its production, and only those who had done it could appreciate the hours, days, weeks and months of thought and labour that were expended on those experiments and observations at high altitudes in the mountains in order that Dr. Swan might be able to give them those results which had only taken a little over an hour to describe. He, himself, had taken some interest in that work, and had recently made observations which might be of some interest, and to some extent supplemented and confirmed what Dr. Swan had told them. An interesting point that struck one was that those observations on mountain altitudes practically entirely coincided with the experiences in aviation, so that one might definitely say that the main factor in all questions of altitude was this deficiency of oxygen or a condition of anoxaemia—deficiency of oxygen in the blood—although, as Dr. Swan had pointed out, one got all sorts of variations in individual cases. One found the same train of symptoms; increase in rapidity of pulse, increase in respiration and a certain amount of increase of blood pressure, whether one did mountain climbing or went up in the air, or into a chamber that was exhausted of air so as to diminish the atmospheric pressure, and, therefore, also the pressure of oxygen. The only difference appeared to be that in climbing one frequently got symptoms, sometimes at 10,000 or 12,000 feet or less, whereas in aviation, he thought, ordinary physically healthy people did not feel any discomfort under about 15,000 feet, and many not until they got into the region of 20,000 feet. Many pilots felt nothing even above that height. No doubt a pilot happy in his mind in regard to ability to control his machine and so forth, and who had little exertion, was in a different position to test his nervous mechanism at that height as compared with a man who was climbing a mountain. Were the blood pressures which Dr. Swan took at the aerodromes taken in the air or after a flight? (Dr. SWAN: After a flight.) It would have been almost impossible to take them in the air at that time, although he had devised an apparatus with which he thought he could get the blood pressures of a pilot in the air by sitting behind as the observer. The two-seater cabin machine, however, came to his aid just as he had got his long rubber tubes ready. He had made flights to 17,000 and 18,000 feet—just about sufficient to feel some sort of discomfort—in those two-seater cabin machines, taking up with him blood pressure apparatus and—his particular stunt—reaction time apparatus. He got some interesting results by taking up pilots, or other physically fit men. He got no definite results from pilots until they got above 10,000 or 12,000 feet. Then there was the usual rise—which varied with different people—of a few points in pulse rate and blood pressure, and when they got as high as the engine would take them (when the machine had reached its ceiling) he got a signal from the pilot and started his final observations. Usually he got a definite slowing in the reaction time at that height as compared with the reaction time of the man on the ground before he went up. In one case there was no change in reaction time at 17,000 feet, but the effort of doing the test caused a considerable rise of pulse rate and fall of blood pressure. On questioning him afterwards, he denied having any unpleasant feelings as a result of the effort. Evidently his (Dr. Stamm's) apparatus was more sensitive than the man's feelings, because a week later he tested him in the altitude chamber and they went up to 15,000. His experience with the chamber was that the test was much more severe than in the air. They only went up to 15,000, but he had to bring the man down quickly without making his observations. On putting him through the reaction time tests, his head dropped forward, he became cyanosed and there was no blood pressure. He gave him oxygen and he was soon all right. It was an interesting fact that his apparatus a week before

had recorded his feeling the stress although he did not appear to be conscious of it. But it was so much more pronounced the following week as to show definitely there was some physical weakness. He did not think there was any proper air sickness, at any rate with aeroplane flying, though he believed there was with airship flying, as Colonel Cave could tell them. He thought that the vomiting one occasionally met with was not of the same nature. It was not due to the movements of the aeroplane, except in cases of severe stunts. In all the tests one realised that the nervous system, as Dr. Swan had pointed out, acted as a wonderful adjusting mechanism. But in one person it might fail at one point, and in another person at another point. In using oxygen in his observations he was very much struck by its efficiency in allaying or preventing symptoms. Not only did one feel so much more comfortable, but the blood pressure went down and the respiration and the pulse rate returned to the normal. Dr. Swan's experiments on the man who was swung over the precipice and the man who was nose-dived were very interesting. The symptoms produced in both cases were, he thought, the symptoms of fright, which were not far removed from those of absence of oxygen. In both cases the organism reacted by a rise of pulse rate, blood pressure and respiration rate. It had been suggested that high flying might be practised for commercial purposes to avoid the weather, and one pictured the idea that a chamber might be constructed in which a man could take up his atmosphere with him—an oxygen atmosphere quite independent of the outside.

Mr. A. E. Berriman, in acknowledging the interesting character of Dr. Swan's Paper, related a personal experience with a view to receiving advice. Accompanied by a friend, he had visited some mines on the Copper Range Railway, perhaps 10,000 feet, and had had occasion to inspect several surface dumps about 10 or 15 feet in height, with fairly steep sides. Quite suddenly, he found it impossible to climb up on to one of these dumps, the chief symptom of physical discomfort being inability to complete a full breath. His friend had no difficulty, but the same man on a subsequent trip up Pike's Peak—14,000 feet—by rail, experienced serious pain in his ears for a long time after the descent, while he (Mr. Berriman) experienced no trouble whatever. He would like to ask Dr. Swan if there was any association between the breathing symptom above described and a similar symptom induced at ground level by overwork, and also by, apparently, indigestion. Was the symptom in every case solely due to a disorganised digestive system, which became acute in one case because of an unsuitable environment, in another because of enfeebled energy, and in another because of an overload of work.

Dr. Swan, in reply, said, subject to Dr. Stamm's agreeing with him, that the trouble was fundamentally due to gas in the intestines, and was susceptible to relief by the use of some simple disinfectant, such as weak carbolic acid, salol, or oil of cinnamon or peppermint.

Dr. A. P. Thurston said he was not a medical man, but he had taken a buff slip along to Dr. Swan on many occasions. He thought he was voicing the feeling of all the old members of the Flying Corps when he said they owed a great debt of gratitude to Dr. Swan for all the work he had done for the Flying Corps, and that they thought him one of the best. He (Dr. Thurston) had been up the Andes and Alps and had flown at great altitudes many times and had never had any feeling of sickness or discomfort when flying. As the result of gas, he now found some difficulty in speaking on the ground, but at above 10,000 feet he could tell a good yarn and chuckle to his heart's content, was perfectly happy, and nothing happened at all. There was a question about coming down suddenly and getting pain in the ears. On one occasion when flying with another fellow, with whom he was connected by telephones to the ears, he did a big nose dive and came down many thousand feet. He felt a tremendous pain in the ears and tried to snatch away the telephones, but the tubes had crossed

and were sealing his ears in some way. He climbed 5,000 or 6,000 feet to relieve the pressure, got the things off his ears and was all right again. He had seen hundreds of people fly without any bad effect at all, or appearance of discomfort. The tonic effect of flying was a very real thing. An hour in the air on a November day was as exhilarating and energising as a week-end at Brighton, and sea sickness, or sickness of any kind was of the greatest rareness. He would like to voice grateful thanks to Dr. Swan for the wonderful work he had done for the old Flying Corps.

Mr. CLARKE asked whether Dr. Swan had noticed any effects of altitude on the senses of smell and taste. In pressure work under water it was well known that there was very little sense of taste or smell. Had Dr. Swan noticed any pronounced effect of the reduced air pressure on those senses? The pain in the ears, which had been mentioned, was much noticed in tunnel work when being released from high pressure, but it was quickly relieved in every case by holding the nose and swallowing. He presumed it was purely mechanical, caused by air trapped in the body.

Major KENNEDY said he had not had many experiences in the upper air, but his memory of those he had had was very good. He had been extremely subject to sea-sickness all his life, and he was full of apprehension when he essayed a flight into the upper air that he would suffer from sea-sickness, but he had found himself altogether free from it. He agreed with the previous speaker as to the tonic effect of an experience in the upper air. The other point he wished to refer to was the physical effect on the ears. In June, 1918, he took a flight in France on a Bristol Fighter. The machine was equipped with a radiator and shutter apparatus in which he was taking a particular interest, and he arranged with the pilot to do some climbing and diving. In a Bristol Fighter one was well able to take a record of the readings of the instruments. They dived from 9,000 to 5,000 feet so steeply that the speed went up to 150 miles per hour. On landing he suffered from what he thought was called throat-deafness. One's voice seemed about 30 or 40 feet away. He tried swallowing and other remedies, but it was 24 hours before it was entirely cured.

The CHAIRMAN said that in order to fly it seemed necessary to carefully consider what should be eaten and drunk, and although we were told we suffered from a whole number of diseases—about most of which he personally knew nothing—yet even when suffering from any or all of these it would be quite safe to fly normally, or at any rate provided stunting and great altitudes were not attempted. It was very reassuring to have this fact definitely stated by so great an authority as Dr. Swan.

Dr. SWAN, in reply to the discussion, said if commercial aviation was going to extend and become universal, machines would have to go up in search of the weather. A fair passage could not always be secured for boats at sea, but they would be able to get information as to the altitude where there would be a fair passage for aircraft. They might have bags of oxygen taking the place of those little basins they saw on cross-channel steamers. He took the fatigue experiments on because that was the last straw the nerve centres could carry. If the experiments had not been done, the vasomotor fault would not have taken place. Altitude sickness was primarily due to the nerve centres. It was due to our want of accommodation and to the lack of oxygen. We should train our vasomotor and respiratory centres to stand a great deal more oxygen-hunger than we did. He had felt the exhilaration that had been mentioned. It was the nearest approach to heaven one could get. He thought the exhilaration was due to the centres all working, one got an increased supply of haemoglobin in the blood, and a certain amount must go to the higher centres, which caused that general feeling of exhilaration. He did not know that one had much means of registering

a sense of smell in an aeroplane. The olfactory nerve was really one of the frontal lobes of the brain, and that made it rather difficult of observation. To avoid the trouble in the ears the nasal passages should be kept clear, especially the back end. Valsalva would help (drawing the breath in, shutting the nose and mouth, and swallowing). When he said everybody ought to be able to fly he did not mean a man with a roaring regurgitating aorta. The disease most dangerous to aviators was syphilis.

The CHAIRMAN asked who was Mr. X., the subject who was swung over a precipice, and Dr. Swan said Mr. X. was himself. Fright did not come in in those experiments and he could not understand them.

A hearty vote of thanks was accorded the Lecturer on the motion of the Chairman.

PROCEEDINGS.

THIRD MEETING, 55th SESSION.

The Third Meeting of the Fifty-Fifth Session was held on Wednesday, November 26th, 1919, in the Hall of the Royal Society of Arts, London, Brigadier-General E. M. Maitland, D.S.O., presiding.

The CHAIRMAN said when Mr. Griffith Brewer did him the great honour to ask him to attend there to take the chair for his lecture, he accepted his invitation with very great gratitude, for two reasons. The first was that he had known Mr. Griffith Brewer for a good many years; they had ballooned together, both with and against each other, at Hurlingham in the old days before the war, and subsequently at Roehampton during the war, and he had always learned to regard Mr. Brewer as one of our universally acknowledged pioneers in this country, and, at the same time, one of our leading technical experts in aeronautics. Mr. Brewer, as his lecture would show, combined the very unusual qualities of being both a very expert and practical balloon pilot, and a theorist and a scientist of a very high order; and those were qualities which they had learnt to admire in Mr. Griffith Brewer very much during the war. It was in 1907, he thought, that Mr. Griffith Brewer was selected by the Royal Aero Club as the representative of Great Britain to compete for the Gordon-Bennett Balloon Race in America, at St. Louis. He was told Mr. Brewer did not win the race, but they would excuse him that. He was sorry to say it was won by Germany, but though Mr. Brewer did not succeed in beating Germany on that occasion he had done a great deal towards beating them since. His name had been very well known to every one of them, not only for his practical ballooning, of which he did a great deal in the early days, but also for his being associated with the very early efforts of the Wright brothers when they first startled the world with their wonderful aeroplane. His second reason for gladly accepting the Lecturer's invitation was that he particularly wanted to take that opportunity of telling those present what Mr. Griffith Brewer had done for the lighter-than-air people—both the kite-balloon service and the airship service—during the war. To explain really what he meant and exactly what Mr. Brewer had done, he must quote some figures. At the time of the Armistice the kite-balloon service numbered some 800 officers and the airship service some 600, and from January 1916, onwards, all those officers passed through Mr. Brewer's hands, when he taught them the elementary lessons in aeronautics and aerostation—the theory and practice of ballooning. During that time, when he was delivering those lectures, over a thousand officers, British, American and Japanese, passed through his hands, and his lectures were so admirable in every way that in America they printed them afresh in Washington and adopted them as standard for training in that country for the United States Navy and Army. That, in itself, was a very fine performance. In addition to that, Mr. Brewer, at his own expense, had devoted his brains and energy to working out problems for them during the war—technical problems—at a time when they very badly needed all the assistance they could possibly get. The results of those experiments had proved of very great value, and Mr. Brewer would tell them about some of them himself. As representing the Air Ministry, and as the senior officer in the lighter-than-air side of the Service, he would like to thank Mr. Griffith Brewer most sincerely for the work he had done for them during the war and the great share he had taken in the development of both the kite-balloon and the airship services. He would now call upon him to deliver his Lecture.

Mr. GRIFFITH BREWER then delivered the following Lecture:—

SOME KITE-BALLOON EXPERIMENTS.

At the end of 1915 I was invited to compile and deliver a course of lectures on the Theory of Ballooning at the Royal Naval Air Station at Roehampton; and for the following three years I attended regularly at Roehampton, delivering two lectures daily and setting examinations for the officers under instruction to pass into the Airship service and into the Kite-Balloon Service

Every encouragement was given for questions to be asked on aerostatic problems after each lecture, my endeavour being to remove as many troubles in the minds of probationers as possible. The trouble which stood out above all others soon became apparent. What would happen to the observers in a kite-balloon in the event of the cable breaking owing to a high wind or to its being shot through? The questions put to me showed this to be the chief cause of anxiety, and it will be seen that the causes for this anxiety were well founded. In the first place there was the danger of the balloon bursting, owing to the rapid rise causing such a great expansion of gas, as to exceed the capacity of the valve to relieve the pressure before a bursting strain had accumulated. Then there was the danger of being carried into enemy territory owing to the length of time required to rise to the maximum altitude of say 8,000ft. before equilibrium had been attained and the balloon could be brought down by valving. And in the case of breaking away at sea, of being blown so far away from the ship that the exigencies of war might prevent the balloon being followed successfully.

If this anxiety could be removed the observers would naturally work better and concentrate their attention on their observations, without worrying about the consequences in the event of the cable breaking.

Owing to the constructional difficulties and weight, the adoption of a very large valve to allow of the immediate discharge of the excess lift was out of the question; besides it would be necessary to discharge from 7,000 c. ft. to 8,000 c. ft. of gas in order to release the surplus lift and cause the balloon to descend, and five minutes' delay in doing this would be prohibitive. Considerable practice and good judgment would also be required in order to know when the right amount of gas had been discharged by valving.

If some means could be devised for immediately releasing the surplus lift in the event of the cable breaking, the balloon could then be brought down from the captive altitude without allowing it to rise to twice or three times that altitude before commencing the descent.

The problem therefore resolved itself into ascertaining the best method of placing at the pilot's disposal the power of suddenly releasing about 500lbs. of lift. It was obvious that the sudden discharge of 7,000 c. ft. of hydrogen would effect this result, but that amount of gas having been discharged, the remaining 23,000 c. ft. must be retained in order to safeguard against a dangerous speed of descent.

A Drachan balloon was placed at my disposal by the Admiralty for the purpose of the experiment, on the understanding that the alterations and experiments should be made at my expense and no indemnity for risk attendant on these experiments would be given.

The alterations to the balloon were then made, and consisted in dividing the balloon into two compartments by means of an internal diaphragm in the form of a parachute. The aft compartment had a capacity of about 7,000 c. ft. and in this compartment a rip panel was inserted at the upper forward end. A circular aperture three feet in diameter was made in the lower part of the diaphragm in order to permit of the free circulation of the gas between the forward compart-

ment and the aft compartment, and I relied on the lift of the gas preventing the flow of the main bulk of the gas from the fore compartment after the gas should be discharged from the aft compartment.

Fig. 1 illustrates the general scheme of the arrangement.

Before making the experiment I thought it advisable to test the speed of descent which could safely be taken in landing. This was done by hoisting a balloon car up to a branch of a tree and by a releasing attachment, dropping the car a measured distance to the ground. The effect of the drop on a man in the car could then be observed. I tried this experiment first with a fall of four feet, then five feet, and then six feet. This last fall was equivalent to a speed of fall equal to 1,200ft. per minute and I felt satisfied with the result, but did not desire to repeat the experiment from any greater height of fall. Afterwards some younger enthusiasts tried the drop from seven and eight feet, when the Commanding Officer stopped further experimental drops for fear of broken legs resulting.

Two instruments for recording landing shocks were then made as shown in Figs. 2 and 3.

The apparatus illustrated in Fig. 2 consist of a number of one ounce weights suspended by threads in a box. One weight is suspended by a single thread, the next by two threads, the next by three threads, and so on, increasing one thread each until the sixteenth weight is suspended by sixteen threads.

When the box is in the car of the balloon, the shock of landing breaks some of the threads and lets the weights fall to the bottom of the box. The loose weights are then removed and the box is dropped from increasing measured heights until another weight breaks loose. It may then be assumed that the shock of landing was equivalent to the effect of a fall slightly less than the distance of fall in the later test.

The instrument illustrated in Fig. 3 shows a lead weight sliding on a rod and supported by elastic. A cork is threaded on the rod and is normally held in position by friction. On the car landing, the lead weight is impelled downwards and extends the elastic and presses down the cork, which is left behind on the rod when the lead weight returns to its original position by the contraction of the elastic.

The first experiments were defeated by minor accidents. The balloon fitted for me at Messrs. Spencer's works, at Holloway, was inflated with air in the open, and just as we were about to enter it for the purpose of adjusting the valve, a gust of wind caught the balloon and carried it over the fence into the next field, tearing it beyond repair.

The Admiralty then undertook the cost of further experiments. A second balloon was made at "Airships," at Merton, and inflated under cover, so we were able to make the necessary adjustments without risk of damage. It had been decided at the Admiralty that a successful descent should first be made in ballast, before making a descent with passengers. Accordingly the balloon was inflated with hydrogen at Roehampton, and the car was loaded with ballast to represent the weight of two observers. The balloon was let up to 400ft. and released by a slip shackle at the metallic V, and then a long rip cord was pulled—and broke. The balloon sailed away unripped and we chased it in motor cars to the Merstham Tunnel, where it descended undamaged.

The next attempt was more successful. The same balloon was inflated at Roehampton and was let up to about 100ft. The slip shackle was pulled from the ground and the rip cords, of which there were two in case one failed, were then pulled. The balloon rose to about 500ft. and descended quickly, but without apparent violence. The shock indicators showed the force of landing to have been equivalent to a fall of about three feet.

This experiment was deemed sufficiently satisfactory, and permission was given for passengers to be carried on the next trial. Accordingly on the 25th April, 1917, Major Max Spencer and myself were let up from the winch at

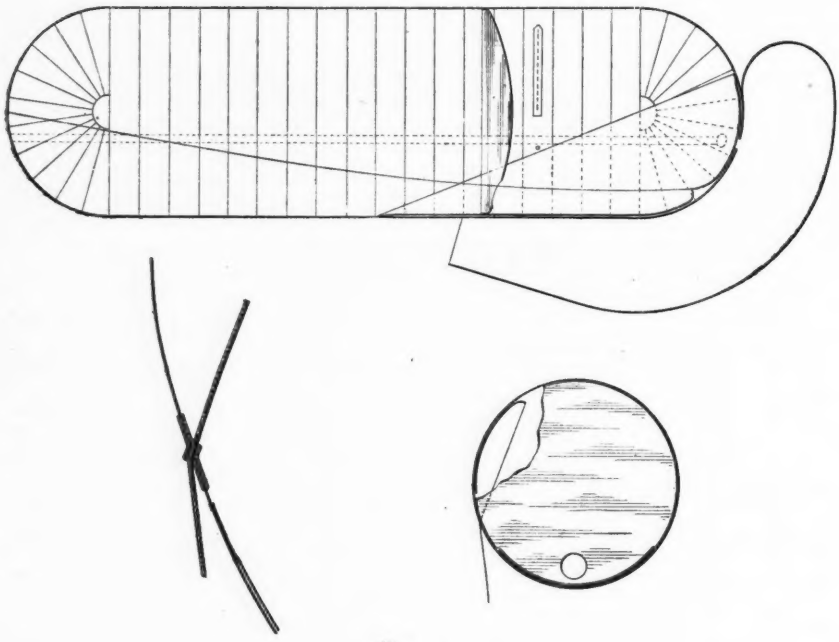


FIG. 1.
Sectional views of experimental balloon.

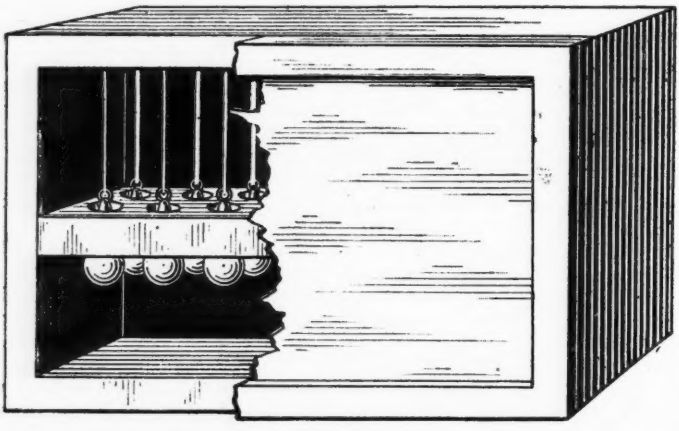


FIG. 2.
A shock tester, crude but reliable.

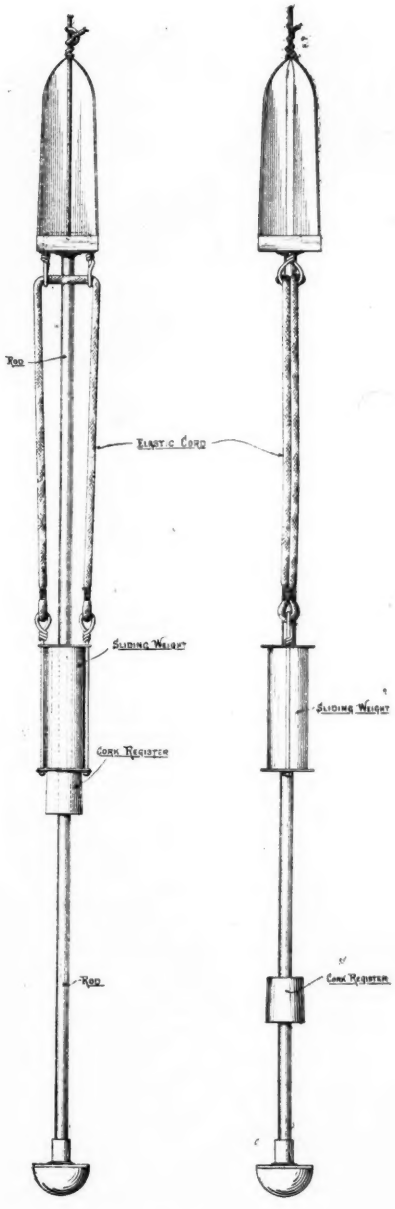


FIG. 3.
Shock tester which may be calibrated.

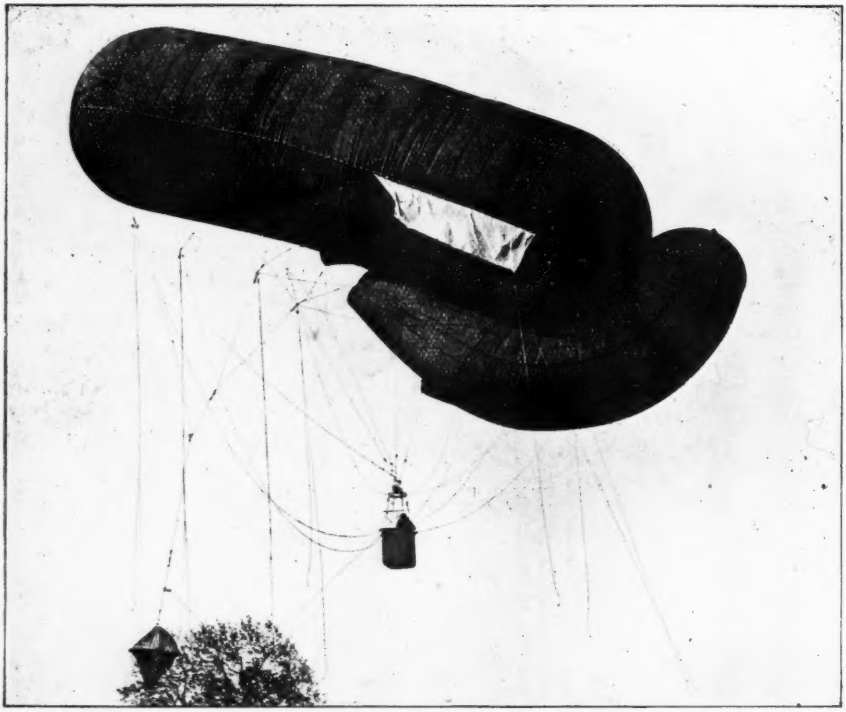


FIG. 4.
Rising on the cable.

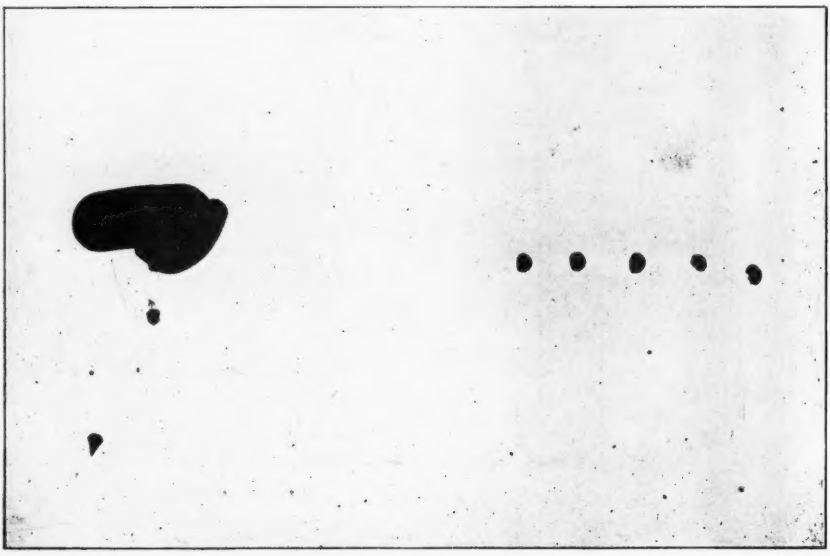


FIG. 5.
Released! Note parachute at top of released falling cable.

FIG. 6.
The first landing.

FIG. 7.
The balloon safely home.

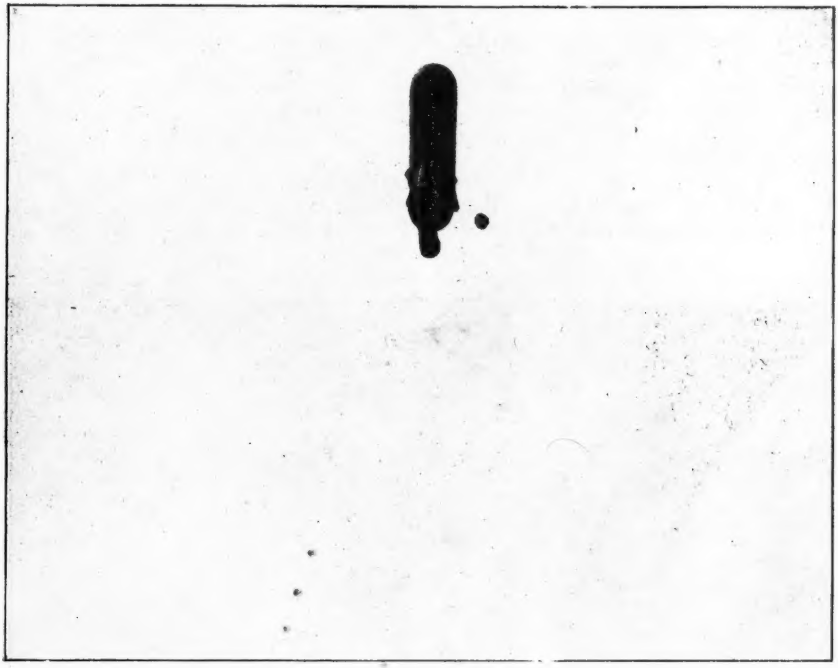


FIG. 8.
Second experiment. Released at 1,200 ft.

FIG. 9.
Third experiment. Released at 2,000 ft.

Roehampton to 400ft. in a wind of 21 m.p.h. On pulling the release, the cable dropped and the balloon rose quickly. Then we pulled the rip panel, and the balloon, after rising about another hundred feet, commenced to descend. No doubt I was somewhat nervous, so finding the balloon descending quicker than we had expected we threw all our ballast and landed in Richmond Park. There was no shock, the branches of an elm tree breaking our fall completely. We were on the ground fifty seconds after breaking away.

The second experiment was made by Major Dalziel and myself on the 4th May, 1917. The balloon was released from its cable at Roehampton at an

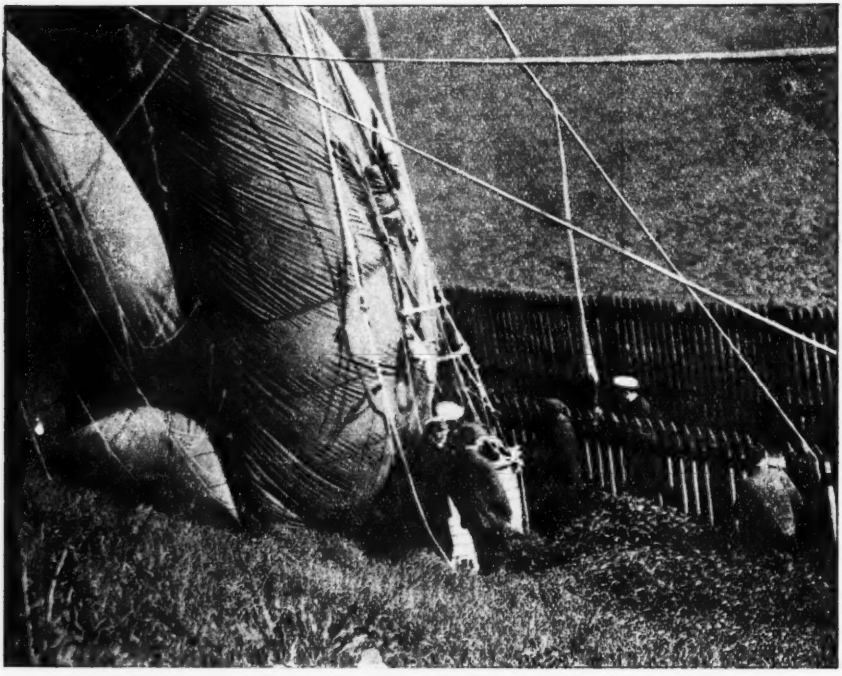


FIG. 10.
After landing on Barnes Bridge.

altitude of 1,200ft. After rising to 1,500ft. the rip was pulled when the speed of ascent had reached 750ft. per minute, and the descent commenced. We had previously decided that unless we were scared by the speed of fall we would throw no ballast but take the bump with knees bent. We certainly came down rather hard, and the wind being rough, we dragged some distance in Richmond Park and through a stream, before we were caught by the men waiting to receive us. The time in the air was 2 minutes 22 seconds.

On the third experiment I was fortunate to get General Maitland to accompany me, and it was at his desire that the break away was to be made as high as the balloon would lift the cable. The cable available was a heavy one and we were let up to 2,000ft., where the balloon refused to lift more weight. At this altitude in a wind of 24 m.p.h. the release was operated and the cable dropped, being checked in its fall by a parachute attached to its top end.

The balloon then free and relieved of the weight of the cable, ascended until the speed-of-rise indicator marked 1,000ft. per minute at 2,500ft., and the rip was then operated. As the gas escaped from the aft compartment the angle of the balloon increased from 45°, its normal free angle, to about 80° when the scoop of the tail approached the car and we could look down into it and see the interior rigging. As the balloon descended she slowly turned round, not quickly enough to give any sensation of giddiness, but continually changing the direction of view. The wind being from the south, our course was towards the Thames, which is only a little over a mile from Roehampton R.N.A. Station. There seemed every chance of the

FIG. 11.
Deflating.

balloon descending in the river, so not wishing to get wet, we put out a bag of ballast at 500ft. and another at 200ft. These saved us from the river, but not from Barnes Railway Bridge, the parachute lines of the balloon catching in the girders of the bridge and making the balloon captive, while the balloon descended on the electric railway. The stationmaster at Barnes Railway Station had previously been warned of the possibility of the balloon descending on the railway, so when he saw it travelling in that direction he stopped the traffic before the actual landing took place and he then stood by the switch "to cut off the current in the event of anything happening." Nothing did happen, however, in spite of the car coming down between the two live rails, rolling first into one rail and then into the other, while the tail sat across the live and wheel rails. The balloon

and the car curtsied up and down, and we had to wait patiently for the breeze to lift us into a safer position. At last after a few minutes, which seemed much longer, we rolled over the rails to the side of the embankment, where our crew, who had run up in the meantime, caught the car and carried it to safety down the embankment and into the field beside the river. There are few insulators better than a balloon car when it is dry, and the car on this occasion was as dry as the stationmaster's remark. I venture to think that we have to thank the dryness of that car for being able at a later date to congratulate General Maitland on his wonderful voyage to America and back in R34.

These three experimental flights seemed to have broken the run of ill luck attending the first experiments, and I was now instructed to fit the invention to

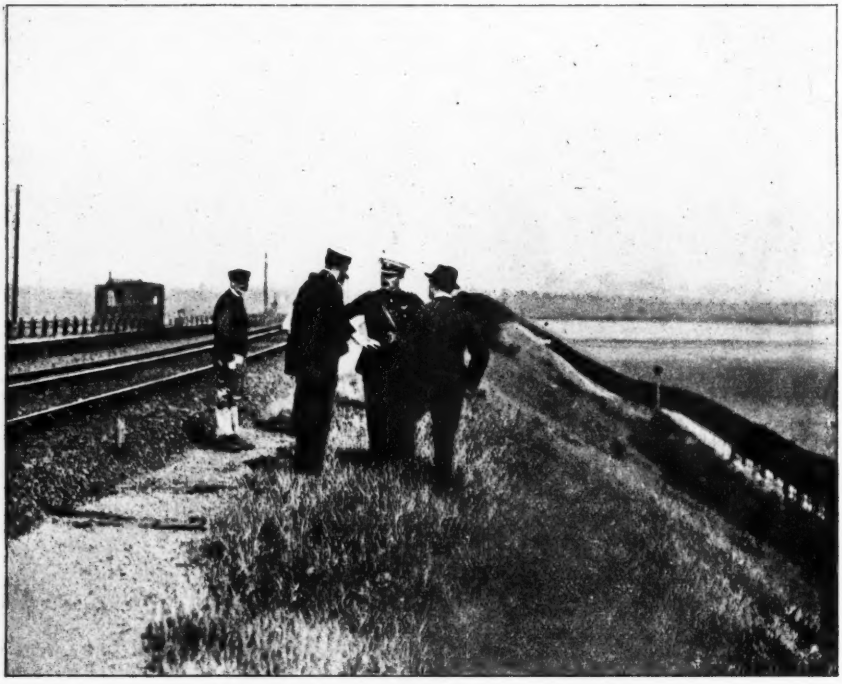


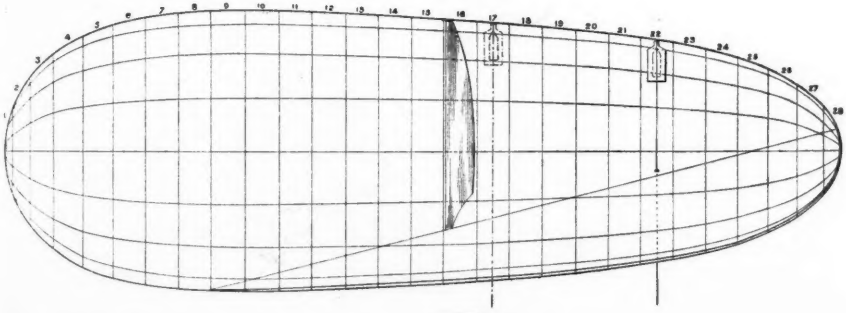
FIG. 12.
Major Dalziel, General Maitland, Griffith Brewer.

a streamline balloon, and, if possible, so arrange that the pilot should have the option, either of descending quickly immediately on breaking away, or of merely bringing the balloon to a state of equilibrium and then to continue as a free balloon. I had already recognised that the quantity of gas discharged depended on the position of the rip panel in the aft compartment, and consequently by having two rip panels, one near the diaphragm and the other near the tail, the option of making a quick landing or of converting the balloon into a free balloon with an open neck, would be at the discretion of the pilot.

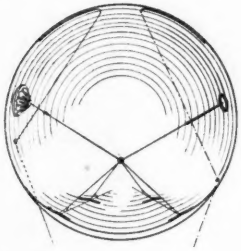
It will be seen (Fig. 13) that the diaphragm has an open arch at its base completely spanning the ballonet. This arch was cut out after the fourth experiment and the diaphragm as first inserted for the third experiment had two three-

foot holes in its base instead. The forward rip panel is close up to the diaphragm, while the aft rip is far back in the tail. In the position drawn in this slide both rip panels would apparently let nearly the same quantity of gas escape, but the change in angle on the balloon becoming free increases as the gas escapes because the tail is robbed of its lift and the lift then becomes concentrated in the forward end of the balloon. The aft rip panel is therefore very low in level, when the balloon is inclined in this way, and becomes equivalent to the open neck used in free spherical balloons.

When the new balloon was completed, it was decided that an attempt should be made to make a free run after checking the ascent. Major Moore accompanied me on this occasion and we dropped the cable at 100ft. By discharge of ballast the descent was checked and a new rise commenced, and then a free run commenced exactly as in a free spherical balloon, except that during the ascents and descents the balloon gyrated slowly owing to the steering action of the deflated tail. A good landing was made fifty minutes later at Rickmansworth.



Longitudinal section of streamline balloon fitted with arched diaphragm.



Cross section showing port and starboard rips behind diaphragm.

FIG. 13.

This ascent was a joy-ride pure and simple, but we were reminded of the war by two incidents. While passing over Northolt Aerodrome an aeroplane flew up to us at 3,600ft., and the pilot cutting off his engine inquired if we required assistance. We assured him that we were quite safe and he then switched on and returned to the ground. After packing the balloon and on our way back to Roehampton an air raid was made over London and we had to return with the head lamps of the car extinguished.

With the object of reducing the weight of the diaphragm I now cut the lower half into the arch form shown in Fig. 13. It was hoped to dispense with the diaphragm altogether, but I hesitated without further trials to do this at the time.

The fifth trial was made in the company of Major Max Spencer, the balloon being first checked after ripping and then ballasted as a free balloon. The

FIG. 14.
Streamline balloon landing tail-down after ripping.

landing was made on Epsom Hills an hour or so later, the maximum altitude reached being 4,000ft. with 100lbs. of ballast remaining.

The trials were then complete, and I was extremely pleased to be informed by the R.N. Air Department that the "Brewer Rip" was becoming part of every naval kite-balloon and was a welcome addition from the pilot's point of view. The use in the navy of the "Brewer Rip" continued for sixteen months, during which I only made one demonstration at Roehampton in the company of Colonel Dunville. I was then informed that the system had been discontinued at the request of the Grand Fleet on account of the additional weight involved in the diaphragm.

A new balloon was accordingly constructed with the two rips as before, but with the diaphragm completely removed, and on the 4th October, 1918, Colonel Mackworth, Director of Balloons, made the first trial with myself in the undivided balloon. The forward of the two rips was now used for the first time and no ballast was thrown to check the drop. The trial was made at Roehampton and the ground contact though fast, was not dangerously so.

It was then decided to make one more trial with the aft rip only, so as to show that the undivided balloon could still make a free run.

Lieutenant-Colonel F. K. McClean accompanied me. The gas in the balloon was old gas taken from another balloon and the car was over-weighted, both of us being above the average weight. We also carried parachutes for the first time. The result of this flight was not what we expected, and was a poor demonstration to my class of probation officers assembled on the ground to view the ascent. The balloon rose sluggishly after being released from the cable and then after ripping fell fast, landing in some trees outside the Roehampton grounds, from which we were ignominiously rescued by our ground crew, who pulled us down from the trees.

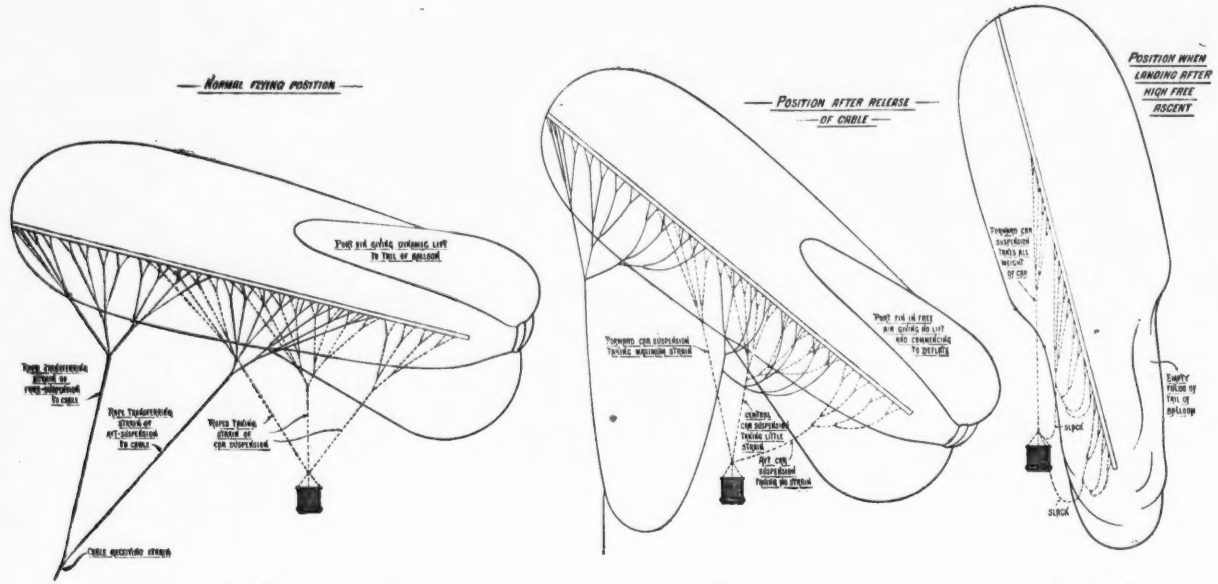
This mishap necessitated a further trial, in which Captain Jones accompanied me. This last experiment was completely successful, a free run of over an hour being made, ending up in the River Mole, near Leatherhead.

The Director of Balloons was now completely satisfied, and once again the "Brewer Rip" became standard for the Navy and for the first time also for Army balloons.

Apart from the actual object in view, these experiments were instructive in many ways. In the ordinary design and use of kite-balloons, the weights and gear are assumed to suit the captive position of the balloon when flying captive on its cable. Accidental break away ascents, although fairly frequent, were always unexpected, and so opportunities to observe the change in conditions in free flight were difficult. By breaking away intentionally, we had opportunities of studying the change in conditions under less trying circumstances, and one thing we learnt was the effect of change in distribution of the car load on the rigging of kite-balloons when free.

The Navy system of running points causes the weight of the car to be carried by the forward car suspensions from eight points on each side of the balloon, *i.e.*, 16 points in all. In the Army balloon the weight of the car is taken when the balloon is free, from one point on each side only, with the extreme probability of the attachments being torn off in succession. Kite balloons should therefore be provided with running points on all the forward suspensions, although running points may be unnecessary in the central and aft points of the car suspension (Models 1 and 2 and Fig. 15).

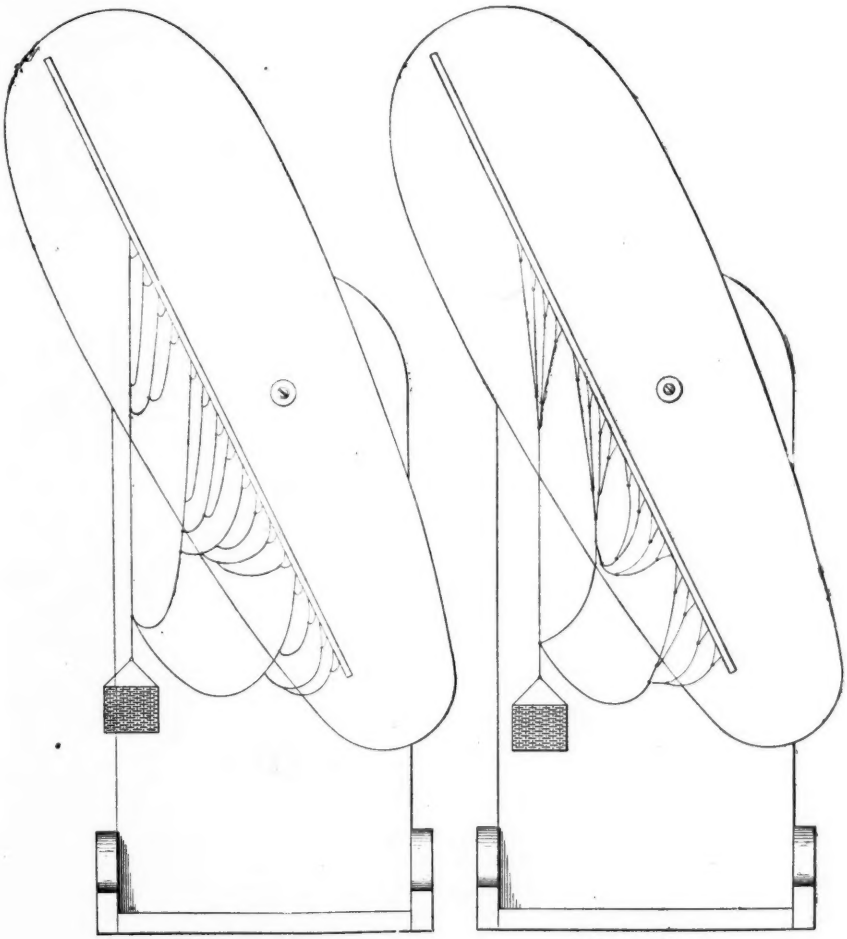
In order to illustrate this danger in kite-balloon rigging, I have here two diagram models, one in which the points of the rigging are fixed, and the other in which the points of the rigging are free to run. In each case the forward leg of the car suspension is divided by the third bridles into two points, from which the second bridles divide into four points, and then by the first bridles into eight points which are attached to the rigging band. In the first model the points of connection are the running blocks used in Naval balloons, and in the second model these


— Normal flying position —
Part of gas bag dynamic lift to tail of ballon
Part transferred strain of fore-suspension to cable
Part transferred strain of aft-suspension to cable
Wires taking strain of car suspension
Cable receiving strain
— Position after release of cable —
Part of gas bag still giving dynamic lift and connecting to outlets
Forward car suspension taking maximum strain
Centre car suspension taking little strain
Aft car suspension taking no strain
Position when landing after high free ascent
Forward car suspension takes all weight of car
Empty folds of tail of ballon
Slack
Slack

Captive. *Free.* *Descending.*

FIG. 15.

points are fixed as in the Army balloon type. It will be seen that when the Navy model is inclined, the strain on the first bridles is equalised by the running blocks allowing the two arms of the bridles to change their proportion of length so as to divide the strain between them, thus allowing the eight points of attachment of the first bridles to each take its proper proportion of the weight of the car. In



Unsafe rigging. *Safe rigging.*

MODELS 1 AND 2.

the Army model the whole of the strain when the model is inclined is carried on the forward bridle alone. In both models the same strength of cotton is used in the first bridles, and the weight representing the car is greater than can be supported by a single thread. By inclining the first model the weight is supported even in the extreme angular position taken up by the balloon when descending after breaking away, but in the second model, a similar inclination of the balloon bringing the weight on to each first bridle in succession breaks them one after

the other and allows the car to fall. I can leave you to imagine the satisfaction we experienced when making these experiments, in observing that when the balloon became inclined to the extreme angle, we were supported by eight of these fine cords on each side of the balloon, instead of by a single fine cord on each side only.

In conclusion, I have to thank the Admiralty for permitting me to describe this invention, which forms the subject of a secret patent which I assigned to the Admiralty. Also I cannot too gratefully acknowledge the help extended by the Director of Balloons and from the Commanding Officer and staff at Roehampton. The officers who have taken part in these experiments deserve ample recognition, for it is one thing to test one's own invention in the air, and quite another to stake one's life on the successful working of another man's invention.

The photographs illustrating this paper were taken by Lady Sybil Grant, hon. photographer to the R.N.A.S. at Roehampton.

DISCUSSION.

The CHAIRMAN said they had listened to a very interesting lecture, and he hoped it might be followed by an equally interesting discussion. Unfortunately, the war came to a rather sudden conclusion and this very valuable innovation which Mr. Griffith Brewer had introduced into the kite-balloon service did not get the opportunities of being tried out both in the Army in the field and by the Navy at sea which they would have liked it to have had. At the same time they, the kite-balloon officers, were convinced that it solved the problem of saving the balloons from getting lost on land in the German lines and at sea by drifting far away from the convoy with which they were working. It was during the time Colonel Delacombe was commanding at Roehampton that these experiments were carried out. He was succeeded by Colonel Dunville, and the experiments were carried on to a further stage. The kite balloon service owed a deep debt of gratitude to Colonel Delacombe for piloting the invention through a difficult and critical state in its development.

Lieutenant-Colonel DELACOMBE said his remarks would be personal rather than technical. General Maitland started the idea of kite-balloons in this country, and but for his prompt action with the Admiralty in the early days England would not have had the real benefit of kite-balloons for a year or eighteen months later than the time when they had it. All those present knew Mr. Griffith Brewer, General Maitland and Colonel McClean pretty well. Mr. Brewer was his mentor in aerial matters and it would be a waste of time for him (Colonel Delacombe) to start a discussion, because if Mr. Brewer stated a thing "it is so." But he might tell the members one or two things they had not heard. When General Maitland spotted a Drachan balloon on the borders of France and Belgium he tumbled to the fact that it was a more useful assistance to the observer than the old spherical, which could not stand more than 18 or 20 miles an hour. The Germans were working the Drachan at 25 to 28 miles an hour. General Maitland, who had then the celebrated "Beta" and some spherical balloons, came home, and told the Admiralty he must have Drachans. He persuaded Commodore Sueter to fit out some kite-balloons for working with naval guns and armoured trains, but General Maitland probably had other things at the back of his head. A house for the work was built at Roehampton, and grew into quite a big thing. Balloons were made for Army and Navy purposes—the Drachan first and the Caquot, invented by Captain Caquot, D.S.O., C.M.G., and which was far more

useful, afterwards. Some 800 or 900 officers and 1,400 men were employed on kite-balloon apparatus for Great Britain. Colonel McClean was the godfather of the Naval Air Service. After the first time he (Colonel Delacombe) had the pleasure of going up in a Caquot balloon—he believed he was the first Englishman to go up in one—he met Colonel McClean in Paris and got him to go up on the second trip in a Caquot. Colonel McClean expressed the opinion that it was all right as a balloon, but said if the cable had broken he would have felt much more comfortable. Anyhow, he passed it. From the very earliest time at Roehampton, when General Maitland wanted somebody to teach the young idea how to do free ballooning, Mr. Griffith Brewer came forward and volunteered to do it, and nobody was better able to do it. Right from 1915 to the very end, without even the satisfaction of wearing His Majesty's uniform, and entirely out of his own time, he came down day by day, week by week, and month by month lecturing to young officers. He was delighted that the Royal Aeronautical Society applauded Mr. Brewer. He was one of their oldest members and Fellows, and he had had no recognition, except from those of them who had been able to express their appreciation of his work. General Maitland was very early dissociated from kite-balloons to go on something more important, but he had an important influence on the kite-balloon service. The men believed firstly in him and secondly in the kite-balloon. The members knew the old difficulties of aeronautics and the opposition of the authorities. They experienced that in the kite-balloon service, and it was only General Maitland's personality and the influence he had on the pilots that got the kite-balloons made and used in the Army and Navy, and the loyalty of the men in the Naval Air Service and the Royal Flying Corps enabled the thing to get through and make good. It emanated from General Maitland and was one of the many things he had done for England. There was an officer there who was an able seaman or something in the early days of the aircraft show at Dunkirk and who was picked out with others by General Maitland to start the kite-balloon service at Roehampton. He was sent back to France to work with one of the early sections with the Army. That officer had a devil of a lot of grit and he—with the assistance of another extraordinarily capable officer, who was unfortunately killed afterwards—persuaded the authorities that the kite-balloon was a useful adjunct to the Army. He was alluding to Colonel Byng, who, by his own determination, convinced lots of people who did not want to be convinced that the kite-balloon was a very useful thing to the artillery, and it was used very largely with the artillery on the Western Front. After the proposal had been before them some 18 months the Admiralty decided that kite-balloons would be useful for convoys and for preventing Fritz, who was getting cold feet then, from attacking with his submarines. Rumour had it that after the kite-balloons were introduced the first 137 convoys conducted through the Mediterranean were immune from attack, and rumour also had it that hardly one-half the convoys had crept through from Gibraltar to Alexandria previously without losing one or more ships. If these rumours were true the kite-balloons, which saved a hundred or more ships in their first few months' work out there, had done something for their country.

Lieutenant-Colonel Byng said that kite-balloons at first could only work in a 26-mile wind and fly at about 2,000ft. At the finish they were flying at over 5,000ft. in a 50-mile wind. At first they were not very popular. He would tell a story about Major Geddes, the young officer mentioned by Colonel Delacombe. The artillery headquarters were being shelled by a German gun which was spotted by Major Geddes and which was 19,000 yards (over ten miles) behind the line. They did not believe they were being shelled by that gun, but Major Geddes told them the time of flight of the shell was 45 seconds. He informed them that they would get one in 45 seconds, and the shell burst by their front door. He asked them if they would like to know when the next was coming, but they said, " No, thank you, we are going down in the dug-out." At the artillery headquarters he

(Colonel Byng) was told the next morning that the kite-balloon service could now do anything they liked with them. Another story that might be of interest was that of a balloon which was set on fire by an aeroplane. The officer jumped out at 3,200ft., his parachute did not open, but he hit a tree and ended in a bit of marsh land and only had a slight bruise on his leg.

Captain E. J. WALKER said he could not claim the responsibility for the design of all the details of the kite-balloons. He helped to carry out one or two improvements that they had from people of technical merit. They had seen, that evening, one way in which problems only arose after the balloons had been adopted as a practical proposition, and it was certain that in the early days of kite balloon work failures of breaking away and failures of cables were very real and of fairly frequent occurrence. Mr. Brewer had shown one method by which he was able to land in safety after the balloon broke away, and that, in the nature of things, was the first to be adopted. In the meantime considerable experimental work had been going on, on the more important point of preventing the balloon from breaking away, and before the conclusion of the war they had succeeded in making breakings away a very exceptional occurrence. This matter involved a number of interesting problems on designs of cables for kite-balloons and others in connection with the balloons themselves. The cable problem was new in aeronautics. They had never had to handle cables of such high strength as was required for balloon work, and the work done on that by certain officers had been phenomenal and very important. He thought it would be of value, not only for aeronautical work, but also in general engineering. The Lecturer mentioned the effect of a break-away on the valves, and stated that some 600lbs. of weight was lost, but in addition to that, in a wind, there would be a considerably greater lift than that due to the drop of 600lbs. There was also the lift due to the wind, which, as Mr. Brewer said, was something of the order of a ton, and it would require a considerable amount of courage and nerve to start experimenting with this device when there were such large forces upsetting the balance of the balloon the moment it was free. He thought it was the first paper that had been read on captive balloon work, and no one was more suitable to give such a paper than Mr. Brewer. No one had been more ready to assist in developing technical problems than Mr. Brewer. During the war, when problems had to be tackled, it was not always easy to get together the gear for experimental work and it often took a long time to prepare for a small investigation, whereas the heavier-than-air people had the whole facilities of the country, including the Royal Aircraft Factory, with apparatus for full scale experiments, and therefore it was a point meriting attention that Mr. Brewer had carried out his experiments with such simple apparatus. The box in which threads were broken by a weight showed what was required and it probably enabled the experiments to be got through in a few weeks which otherwise would have had to drift on for months. He did not think the experiments in regard to the suspension of the car when the nose of the balloon was tilted up were altogether conclusive. The forces that came on the balloon rigging were more complex than the simple tension shown on the model. The use of the running reel brought in a disadvantage which was not apparent in that experiment—the considerable weakening which occurred in both cordage and wire when run backwards and forwards an indefinite number of times over small iron pulleys. It was a difficult matter in the aeronautical world to get a sufficient allowance of weight to put on a pulley of sufficient diameter not to interfere with the strength of the cable. There was a big deterioration of strength not only of balloon rigging but also of larger cordage and wire ropes. He did not think that could be completely got over by the method shown, and probably they would have to go along some different lines in the future to get better results.

Lieutenant-Colonel MCCLEAN said he did not know anything about kite-balloons. He was inveigled into one by Colonel Delacombe and into another by

Mr. Brewer. The first was all right as it was calm and Colonel Delacombe did not try experiments on him. The second one they had heard about. Having watched Mr. Brewer's experiments, more as an outsider, and seen the instruments, he thought extraordinary progress had been made by Mr. Brewer, who was always working at his place in Bloomsbury.

The CHAIRMAN said there were some 800 officers in the kite-balloon service when the Armistice came. They were all trained by Mr. Griffith Brewer in the elements of ballooning, but they were all taken on into the kite-balloon service by Major Davison, who might tell them some of the difficulties he experienced.

Major DAVISON said they were extremely lucky in the early days. Entirely owing to General Maitland they were allowed to select every officer and man through the whole force. They had no trouble with the officers and no lock-up for the men. If the men were set to do a certain job during the day the officers could be given a holiday and the work would be done. There were 250 young officers in training at one time. The two things they liked best were Mr. Brewer and his lectures and the ship recognition course. They said they liked Mr. Brewer because he was lucid and human, he was always out for any experiment and he did not care how risky it was—he went up and did it himself.

The CHAIRMAN said Mr. Griffith Brewer had also done an immense amount of work for the airship service, and Colonel Cave-Brown-Cave, who was in charge of experiments and research on the airship side, would, he was sure, give them his views from the airship point of view.

Lieutenant-Colonel T. R. CAVE-BROWN-CAVE said he did not feel qualified to discuss the Paper, as his experiments with the kite-balloon were extremely limited. He would like to ask Mr. Brewer what degree of consistency he got in the results of the experiments with his instrument for determining the magnitude of the shock on breaking the cable. He felt considerable satisfaction that the running rigging was provided in the Naval balloons, at any rate, and if, as the Lecturer said, it was a means of avoiding an almost certain danger of the riggings parting one after the other, even if it involved a certain amount of risk, he thought it was justified. Presumably, the action of the running rigging was realised by the designers of balloons other than the naval ones, but he thought the naval scheme was probably the right one. Looking at the work as a whole he was struck by the simplicity of the device, and that simplicity in its completed form was an indication that it was a really good invention.

Mr. GRIFFITH BREWER said, in reply to the discussion, it was true the running points chafed the cordage and it required renewal earlier than fixed points, and he believed that was why the Army balloons were changed to fixed point rigging. His objection to fixed point rigging was met by putting in all running points on the forward car suspension. That meant a set of seven running points on each side of the balloon, and it was only on those points that it was necessary to make the change and still obtain the full advantage of the safety. Captain Walker referred to the complex character of the strains brought on the gear, but when the car was suspended entirely on the forward car suspension the balloon was in perfect rest, because it was a free balloon. It was merely a matter of the dead weight of the car on that rigging. If that would not break the rigging one could possibly go within a factor of safety of $1\frac{1}{2}$ or 2. With regard to the instrument in which threads were broken in the shock testing experiments, when a number of threads were broken it took a long time to put them back, so he could not give the instrument any character as to accuracy.

The CHAIRMAN moved a vote of thanks to the Lecturer and Major-General

R. M. RUCK moved a vote of thanks to the Chairman and spoke in appreciation of the work done by the kite-balloon section and by Mr. Griffith Brewer, who, he said, had always been a valuable member of the Society. Everybody knew the good work General Maitland had done during the war, and he moved a hearty vote of thanks to him for presiding at that meeting.